Comments and Discussion

William Poole: Mishkin's paper provides a thorough examination of efficient-markets theory and many useful ideas on its implications for macro models and monetary policy. His empirical results replicate and extend previous work on efficient markets. His work is careful and thorough. As far as I can see from reading the paper, he has been extremely careful in his treatment of the data and in his statistical analysis.

Mishkin's empirical work uncovers only one puzzle. In equation 29, when he regresses the return from holding common stocks on the three-month treasury bill rate, he finds a negative coefficient rather than the positive one predicted by the theory. The efficient-markets model, of course, predicts that, except for differences in returns due to risk and liquidity premiums, returns should be equalized on all assets; but Mishkin's equation comes up with the result that when the treasury bill rate is high, the rate of return expected on stocks is low.

To understand that equation, suppose that the bill rate is relatively high at 8 percent. The quarterly rate of return on bills in decimal form is then 0.02. A bill rate of 0.02 times the regression coefficient of about −5.0 is −0.10. Add to this figure the constant term in the equation of 0.08, and a per quarter expected return is obtained from holding common stocks of −0.02, or −8 percent per year. Similarly, when the bill rate is relatively low—say 4 percent per year or 0.01 per quarter—the expected return on common stock is 0.03 per quarter or 12 percent at an annual rate.

The puzzle is how the expected return on common stocks can be negative when treasury bills earning a positive rate of return can always be held. A possible explanation for this result depends on the existence of transactions costs. The time series of changes in the bill rate displays negative serial dependence. Therefore, whenever the bill rate is abnormally high, say, 8 percent, the rate is on average expected to fall in the future. The expected return from holding bills for two quarters—either one six-month bill or two successive three-month bills—will be less than the abnormally high three-month bill rate of 8 percent. Because of transactions costs, it is not profitable to sell stocks at time $t - 1$ with the expectation of buying stocks at time t when the one-quarter return on stocks between t and $t + 1$ is expected to be higher. Transactions costs of trading common stocks require that the planned holding period be longer than three months.

If this argument is correct, running the equation on semiannual rather than quarterly data should yield a larger regression coefficient. Perhaps the coefficient would still be negative but not as much as in the quarterly regression. Indeed, under this argument, the longer the holding period analyzed, the closer the coefficient should come to 1.0. I have no way of knowing whether the transactions costs explanation is correct, but it is at least a possible explanation for the puzzling outcome.

The implications of Mishkin's results for macro models are brought out most clearly in his table 1. Consider, for example, the predicted return from holding bonds with a 20 percent return per year for the fourth quarter of 1976 calculated from the term-structure equation using the forward rate measure of expectations. This relatively high return indicates that the term-structure equation predicted bond prices would rise, or bond interest rates would fall, over the quarter.

Mishkin interprets this result as showing that the term-structure equation is deficient. If we were to examine the term-structure equation residual at the end of such a quarter, then, he argues, we should see the actual interest rate on bonds higher than that predicted by the term-structure equation. The realized bill rate at the end of the quarter may differ from the forward bill rate prevailing at the beginning of the quarter; but if the forward rate is an unbiased forecast, then on average in such situations the term-structure residual at the end of the quarter should err as indicated. This proposition could be tested by seeing if the end-of-quarter residual of the term-structure equation that is predicted at the beginning of the quarter by the efficient-markets model in fact is positively related to the realized residual.

According to the argument just discussed, an observation such as that for the fourth quarter of 1976 reflects a term-structure equation prediction for the interest rate on bonds at the end of the quarter that is too low. But it is also possible that the term-structure prediction for the end of the quarter is correct and that the problem is that the bond interest rate at the beginning of the quarter is erratically high because, for example, of heavy placements of new bond issues. This proposition would be supported by evidence that the volume of new placements, or trading activity, or some other variable known at the beginning of the quarter could explain the high expected yield over the quarter. It would also be supported by evidence that the term-structure prediction for the end of the quarter calculated from information available at the beginning of the quarter is a more accurate predictor than is the efficient-markets model.

Mishkin's evidence, as well as that of Phillips and Pippenger in the paper cited by Mishkin, does not support the second possibility. Mishkin did not test for all possible variables, such as trading volume, that might conceivably be related to the bond interest rate, but he is hardly to be faulted for not playing this game. There is an indefinitely large number of such variables and, without hypotheses as to which variables might appear, he would have to engage in a fishing expedition of the worst sort. His purpose was not to find such variables but to show that the standard term-structure equation cannot be adequate. If the bond interest rate really is affected by the volume of new placements, then that variable should be added to the term-structure equation.

Having said all this, and agreeing with Mishkin's basic point, I should add that the errors of the term-structure equation may not be serious. Assuming that the 20 percent expected return in table 1 reflects an excess return of 15 percentage points, the term-structure equation is generating a prediction of the bond yield that is in error by about 40 basis points. Such an error is not trivial, but it does not seem catastrophic. Investment equations are not so sensitive to the long-term interest rate that missing it by 40 basis points—especially if the miss does not last too long—will have a major effect on a model's GNP predictions. If I were a model builder and expected that future monetary and fiscal policies would be fairly similar to those of the past, I suspect that Mishkin's table 1 might convince me to leave the term-structure equation alone and to concentrate my efforts on other problem areas, such as the money-demand equation.

Mishkin's analysis of the term structure makes it clear that the equation is theoretically indefensible for all simulation experiments involving monetary and fiscal policies that are fundamentally different from the policies in force over the period of estimation. His discussion of the policy implications of the efficient-markets model elaborates on the story.

Mishkin argues that lags in policy effects should be shorter than the lags predicted by standard econometric models; indeed, in the efficient-markets model there can be no lag at all in financial markets because new information, including that of a policy change, is immediately reflected in changes in securities prices. The magnitude of the effects of changes in monetary policy on securities prices will exceed that of the standard prediction when changes in policy are perceived as permanent, and will be smaller when the policy changes are expected to be especially temporary. A substantial money stock bulge that is expected to be reversed may affect the bill rate but should not affect the long-term bond yield as much as the standard term-structure equation predicts. The key point is that the impact of a policy change in a particular quarter cannot be determined without specifying expectations concerning future policy.

Mishkin draws some conclusions that appear to contain both good news and bad news. I see only the bad news. Suppose policymakers observe bond rates changing by more than predicted by the term-structure equation. How can they possibly know whether the change reflects market expectations concerning future monetary policy or some other factor? I submit that, without direct and reliable data on expectations, there is no way to sort out expectational factors from other factors, including those reflecting policy changes.

At the end of the paper Mishkin seems to be struggling to maintain that macroeconometric models are of some value although not as much value as previously thought. His last sentence reads: "Incorporating efficient-markets theory into macroeconomics does not lessen the need for policy-oriented and basic research, but it does require some redirection of thinking." Of course, the efficient-markets theory does not lessen the need for policy-oriented research, but it does require more than "some redirection" of our thinking. I would have preferred that Mishkin end with another sentence that appears early in his paper: ". . . efficient-markets theory implies that the macroeconometric models currently used for policy analysis and forecasting are deficient in a fundamental way." Until a way is found to incorporate in econometric models the expectations of economic agents about the future course of policy, we should be very cautious in using these models to provide predictions about the likely effects of alternative policies. The problem *is* fundamental and it requires more than a minor redirection of our macro analysis.

Franco Modigliani: According to Mishkin, his paper is intended to show "that current procedures for evaluating policy and forecasting with macroeconometric models are inconsistent with market efficiency in bond and stock markets" and that "incorporating efficient-markets theory into macroeconomics . . . does require some redirection of thinking." In my view, both these conclusions are unwarranted and reflect a basic confusion between explaining a variable and forecasting its change as well as between conditional and unconditional forecasting.

The objection to the "current procedures" is that the equations used in existing models explain such financial variables as the stock market and the long-term interest rate, and their change, as a function of past "publicly available information." Such a formulation is supposedly inconsistent with "efficient-markets theory" because according to the theory, the change in these variables must be uncorrelated with past publicly available information. My first concern will be to show that this line of reasoning is faulty—it must be because one of the equations criticized is the Modigliani-Shiller term-structure equation, which relies squarely on efficient-markets theory.

To begin, neither stock nor bond yields are, strictly speaking, martingales, and this holds a fortiori for the value of equity and the bond rate (see below). But forget this technicality for the moment and assume that, say, the market value of equity is a variable with the property that its percentage change is a martingale. It should be obvious to an economist that this single property cannot and does not provide an explanation of what determines or explains that market value. If we try to understand its determinants, we will be led to such variables as the expectations of future profits and of future interest rates. And each of these expectations will clearly be shaped, in large measure, by the past history of profits, interest rates, and other variables. Thus the market value of stock will finally be explained by current and past profits, interest rates, and so on. Similarly, the bond rate will depend on expectations of future short-term rates and risk premiums, which will be influenced by current and past interest rates, inflation, and so on.

But then the change in market value, because it is the difference between a function of current and past values and a function of past values only, must be a function of current and past values of publicly available information. Mishkin would want us to believe that this conclusion must be rejected because, by efficient-markets theory, that change is a martingale and hence cannot be correlated with past values. Intuition tells us that his reasoning must be in error because dependence on the past and (near) martingale properties of the change are such obvious characteristics of a variable like the market value of equity that there should not be a need for choosing between them. And indeed, one can readily show that there is no inconsistency.

The easiest way to convince Mishkin and the reader is to rely on Mishkin's own equations and tests. Mishkin has assured us that the bond yield series passes the test of a martingale. Yet his equation 35 shows that the yield depends on the current short-term rate and a measure of the "expected rate" for period t, $ERAR_t$, which according to equation 20 is given by a distributed lag of *past* short-term rates, much as it was in the Modigliani and Shiller model (even though Mishkin's equation is not a very sensible one). Thus substituting 20 into 35 shows that the yield is a function of the current and lagged short-term interest rates. Furthermore, if we solve the yield equation for the bond rate, this rate will be found to depend on the lagged bond rate and on these short-term interest rates. And if the lagged long-term rate is eliminated by recursively expressing it in terms of previous long- and short-term rates, one ends up with a form similar to that of Modigliani and Shiller, in which the long-term rate is explained by a long distributed lag of the short-term rate alone.

Similarly, from 36 we find that the stock return—and hence to a close approximation the change in market value and the market value itself—is a function of the current and lagged long rate, and the equation at the top of table 4 suggests that it depends also on the bond rate lagged many periods. Furthermore, the MPS equation, as well as some work in which I am presently engaged, suggests that the equation of table 4 leaves out many other relevant current and past variables. To conclude, then, the fact that a variable has the characteristic that its first difference is a martingale in no way excludes the possibility that it, as well as its first difference, may be a function of current variables and variables with substantial lags.

Because this conclusion may appear puzzling in the light of Mishkin's paper and has generated so much confusion, it may merit some further comment. First, from a formal point of view, the puzzle may be clarified by noting that the martingale property does not exclude the possibility that the first difference (and hence the level) is a function of current *and* past variables, since the only restriction required for a martingale is that the first difference should not be correlated with past variables in a regression that excludes all current variables. Indeed, as can be seen from tables 3 and 4, once the current variable is dropped (*RTB* in table 3; *RCB* in table 4), neither bond nor stock yields are significantly correlated with past variables.

Quite generally if S is a variable for which ΔS is a martingale, and $f(\cdot)$ is a proposed "explanation" of S ($S = f(\cdot) + u$, where u is an error term), the condition that the explanation is consistent with the martingale property of S is not that $f(\cdot)$ contains no lagged arguments but merely that $\Delta f(\cdot)$ itself (and, similarly, Δu) is a martingale. This result can be illustrated with reference to the Modigliani-Shiller model of the long-term rate. In this model the long-term rate is a (weighted) average of expected future rates, which the market is assumed to forecast from the past history of short-term rates, inflation, and so on. This formulation implies that the change in the long-term rate can be expressed, as in Mishkin's equations 20 and 35, as the sum of two components. The first is systematic and anticipated and reflects the capital gain appropriate to insure that the expected holding yield is equal to the short-term rate plus the risk premium. The second reflects the difference between the expectation of future rates held now and the expectation held in the last period. That difference between two functions, each of which involves many past variables, turns out to be a martingale. It is in fact determined by the difference between the actual short-term rate in the current period and the forecast of that rate made in the previous period, a forecast error that, by construction, is uncorrelated with the past history of interest rates, inflation, and so forth. The specific equation assumed to generate the expectations of future rates used by Modigliani and Shiller is quite different from Mishkin's, and while I do not know whether it performs better, I suspect that at least it was thought out more carefully, allowing among other things for the effects of inflation.

This clarification of the Modigliani-Shiller structure is also helpful in understanding why the laborious exercise carried out by Mishkin in table 1 to discredit the Modigliani-Shiller approach basically misses the point. Rather than use the Modigliani-Shiller equation, Mishkin estimates another equation that is of rather questionable specification because it disregards the role of inflation and has unacceptable steady-state properties. (The sum of the interest coefficients is 1.3.) But what is most serious is that, to obtain the predicted bond rate of the next period needed to calculate his expected yield, he substitutes in the bond-rate equation 15 or 16 an independently obtained forecast of the short-term rate for the next period. This procedure is unwarranted because a forecast of the short-term rate for the next period is already embedded in a properly specified and estimated Modigliani-Shiller equation, and that forecast is the only one that is consistent with that equation. More precisely, the yield expectations implied by the Modigliani-Shiller framework are neither the figures of the first column nor those of the second column of table 1, but rather are those in the third column plus the risk premium (which itself is a variable in the Modigliani-Shiller equation). From this yield one can infer the expected bond rate for the next period implied by the model, and hence, finally, the expected short-term rate.

Thus the only meaningful exercise Mishkin could have carried out would have been to compare the expectation of the next short-term rate implied by his equation 20 with that implied by his version of the Modigliani-Shiller equation, although I doubt that even this exercise would be worthwhile given his cavalier reestimate of the Modigliani-Shiller equation. Note finally that his huge and extremely volatile yields are perfectly consistent with the relatively small difference in the expected short-term rates of the next period, because these yields are about 40 times larger than the difference between the current and expected bond rate.

Mishkin's discussion of economic model forecasting illustrates his inability to appreciate the distinction between the requirements of unconditional forecasts and those of conditional forecasts, which are relevant for agents who may possess (or believe that they possess) superior knowledge of relevant future variables, or who, like the government, may be in a position to influence those variables. For a person having no special information or control over the future, the best forecast of the next value of a variable S with martingale properties is clearly the current value (up to some constant). The knowledge that S_t can be explained by $f(x_t, x_{t-1}, \ldots)$ would be of little value to him because S_{t+1} depends on x_{t+1}, which he cannot predict any more accurately than the forecast already implicit in S_t. It is, presumably, in this spirit that Mishkin suggests that builders of econometric models might wish to adopt his equation 40, which states that the bond rate from here on is equal to today's bond rate plus an unexplainable error. But this equation is clearly worse than useless for policymakers—it is actually misleading because it implies that the long-term rate would be unaffected by anything his policy might do to the short-term rate.

What the policymaker needs to know for evaluating policy is whether and how S might respond to some variable x that he controls directly or that he might affect through other policies. Policymakers, therefore, need an estimate of the function f because, if S_t actually does depend on x_t, then ΔS is not a martingale. Hence I would confidently predict that, despite Mishkin's plea, econometric model builders will not (and should not) adopt his equation 40. They will instead appropriately continue to endeavor to establish what actually determines S—martingale or not—and in particular how it responds to policy variables.

Mishkin's analysis does offer something more constructive than equation 40 to the policymaker. For instance, 34 can provide an estimate of the response of the long-term rate to a contemporaneous change in the short-term rate; even better, 35, together with 20, makes it possible to estimate a whole future path of the bond rate as a function of the future path of the short-term rate. Before accepting these estimates the user would be well advised to compare the quality of conditional forecasts produced by these equations with that of other existing formulations.

I am happy to report that there is one issue on which I find myself in agreement with Mishkin, though it is not a new issue, nor, by now, a particularly controversial one. It is the proposition that the coefficients of equations explaining a financial variable like the value of equity or the long-term rate may not remain stable over long periods of time. Indeed, such variables are in the nature of long-term purchase or rental contracts, whose value, at any point of time, must of necessity reflect expectations of future variables over long periods of time. (The fact that for highly liquid assets the return is fully determined by the expected value of the asset at the next point in time does not change this conclusion because the current expectation of the next-period value depends on the expectation of variables beyond that period.) Therefore, in attempting to explain such financial variables, one must somehow model expectations that can only depend on current and past variables. But clearly the parameters of such expectational relationships do not reflect unchanging technological laws or even tastes; such parameters might be more appropriately described as complex reduced forms reflecting, among other things, psychological elements and the public's perception of the way in which policymakers may be expected to respond to evolving events. Accordingly, such coefficients are in principle subject to gradual or sudden change, and may, in particular, vary with major changes in policy rules or policy styles. Contrary to what one might infer from Mishkin and some of the literature on rational expectations, the instability of the relevant parameters is not an inexorable law of nature, but merely a possibility depending on circumstances and subject to empirical verification. In the case of the Modigliani-Shiller term-structure equation, for example, the evidence suggests that the underlying expectational mechanism has been quite stable. Thus the equation fitted to the period from 1954 to 1966 for the MPS model has been found to perform remarkably well for the next ten years.

At the same time, anyone using the equation must be on guard for circumstances likely to result in a change in structure; for instance, an event like the introduction of price and wage controls, as in 1971, would call into question whether price expectations could continue to be approximated by the same distributed lag of past inflation, and at the very least would suggest an increase in the margin of error of the equation. Similarly, suppose the Federal Reserve were to engage in an entirely new (and preposterous) policy of announcing a change in the treasury bill rate to be maintained forever (something which, of course, could be done only once in history and if there were strong reason for believing that the public would be gullible enough to believe it). Then any sensible forecaster asked to predict the consequences of such a policy would run precisely the simulation that Mishkin has performed and which for some curious reason he likes to label "efficient-markets simulation." I would bet that the typical model builder would have such a simulation already in his drawer because he has to analyze the dynamic characteristics of his model system, and thus he is likely to have simulated at some time or other the response of that system to a change in the long rate.

Despite all my criticism, I feel that we should all be grateful to Mishkin for a provocative paper that has forced us to think through and clarify some of the apparent puzzles associated with efficient-markets theory. I, at least, feel much more confident after this exercise that the efficient-markets theory will *not* require much significant redirection of my thinking.

Frederic S. Mishkin: There really is less disagreement between my view and Franco Modigliani's than may first appear. Modigliani is quite right that efficient-markets theory does not imply that a long-term security's return (or, almost equivalently, the change in its market value) is unrelated to current and past values of a variable such as interest rates. Yet it does imply that the security's *excess* return is uncorrelated with past information, taken alone. This is clearly brought out in the paper in the efficient-markets model of equation 21 and its empirical counterparts, 34–36. The efficient-markets model states that the return on a long-term security (neglecting possible movements in the equilibrium return) should be correlated only with surprises, or more precisely, with the deviations of actual values from the optimal forecasts, $X_t - X_t^e$. However, as is the case in 34–36, these surprises can be estimated as a function of current and past variables. Indeed, 35, where the bond return is a distributed lag of current and past short-term interest rates, is totally consistent with the view inherent in the Modigliani-Shiller term-structure equation. Of course, that might have been expected because Modigliani and Shiller's seminal paper provides important evidence that lends support to bond-market efficiency.

What is of critical importance to macroeconomic analysis in this paper is the idea that, as an approximation, only surprises influence the valuation of long-term securities. This in no way denies the usefulness of searching out the factors that influence the prices of these securities. The Modigliani-Shiller term-structure equation, as well as dissimilar ones (such as 35 of this paper), provide information that a policymaker would find valuable, indicating how long-term rates react to a surprising change in short-term rates. Even when financial market efficiency is imposed on a macro model, this allows us to ask the question: what effect on the economy results from certain shocks? There are simulation methods for exploring this type of question that are consistent with financial market efficiency, and they are applied in the paper cited in note 53.

What market efficiency tells us, however, is that we must be quite careful in our use of macroeconometric models and that policymaking might be a trickier business than is sometimes conceded. The following problem arises. Even if estimated equations predict that a surprise shock to a variable such as the short-term interest rate will have a particular effect on long-term rates and hence on the economy, the policymaker cannot assume that, when he applies a shock to this variable, it will automatically lead to the predicted effect. The actual effect of the policy action will depend on market expectations, and the policymaker should be aware of that source of uncertainty about the results from his actions.

This paper also criticizes the use of term-structure equations for forecasting. The experiments in table 1 tell a simple story. The existence of error terms in term-structure equations indicates that the market, in making forecasts, uses information beyond that included in these equations. In fact, some of this information is probably so subjective that it cannot be included in an econometric specification. Since these term-structure equations must neglect this type of information that is available to the market, their forecasts may be suboptimal. It is in this sense that such forecasts are inconsistent with market efficiency, and this is why the results in table 1 should not be surprising. William Poole's calculation of the magnitudes implied by table 1 for the difference between the efficient-markets forecasts and those from the term-structure equation are reasonably accurate; and he is justified in saying that, from a forecasting point of view, other sectors of macroeconometric models may have more serious deficiencies. On the other hand, using an alternative procedure consistent with market efficiency might be worthwhile because it is so cheap and easy. One extremely crude alternative is the approximation of 40, where the future long-term rate is predicted to be unchanged from today's rate. Note, however, that this paper never suggests that an equation like 40 should be used in a simulation context; that use would contradict the theoretical perspective that both efficient-markets theory and this paper put forward.

The time and effort spent in analyzing the implications of efficient-markets theory has led me, at least, to redirect much of my thinking. I agree with William Poole that the problems that market efficiency poses for macroeconometric models are fundamental. Efficient-markets theory does require a major redirection of macroeconomic analysis.

General Discussion

Robert Hall found convincing Mishkin's argument that, although the standard term-structure equation could predict the effects of a *typical* policy measure, it was inadequate for forecasting the effects of a *particular* policy action, especially the effects of an innovative one. He stressed, however, that Mishkin had failed to provide an alternative way of closing the model when a policy innovation occurred. Hall noted that one procedure was to use the short-term rates predicted by the model itself to generate long-term rates consistent with rational expectations.

William Branson and Rudiger Dornbusch traced the steps required, in principle, to predict the effects of a particular policy action. First, one needs to model the expected or endogenous policy response; next, the "surprise" in policy can be calculated as the difference between the actual and expected action; finally, a reaction function of the market is needed to translate the surprise into altered expectations for the future. Branson felt that Mishkin was correct in arguing that major innovations in policy would instantaneously shift the entire yield curve of interest rates through the reaction function. But he felt that standard term-structure equations were useful shortcuts for assessing the effects of routine policy measures. Christopher Sims also saw some usefulness of the standard term-structure equations. He pointed out that, since these equations are specified in level form, they give greater weight to long-frequency changes, which are pertinent for tracking the effects of a policy change over several quarters. He added, however, that for tests of market efficiency, the greater weight given to the shorter frequency changes in the first-difference formulation might be preferable.

Michael Wachter believed that, while the coefficients were unlikely to be stable, people would change their expectations about policy only gradually. Hence, the equations would be usable although they would need to be reestimated from time to time. He also suggested that Mishkin might explore how fast the coefficients have changed in response to identifiable policy changes in historical episodes.

Martin Baily felt that the paper had overemphasized policy innovations and thus exaggerated the inadequacy of distributed lags. Discretionary monetary policy has been applied continuously for a long time—sometimes wisely, sometimes badly, sometimes surprisingly, sometimes routinely. The distributed lag equations that summarize typical experience should provide a fair amount of insight on the economy's response to policy.

William Poole countered that optimal control solutions derived with the MPS model suggest monetary policies that are very different from the way the Federal Reserve has actually behaved. Thus the historical experience reflected in the coefficients of the model would not be suitable for forecasting the effects of an optimal control strategy of policy. Poole therefore doubted the usefulness of such exercises.

Baily also emphasized that "expected" policies had an effect before they were enacted: they should not be regarded as ineffective. Mishkin agreed that efficient markets could make policy more effective by reflecting expectations about policy promptly.

Benjamin Friedman noted that the paper and also the dialogue between Mishkin and Modigliani had focused on two alternative ways of tracking bond interest rates—the term-structure equation with distributed lags, and some variant of a random walk model. Friedman pointed out that a third alternative was the explicit modeling of supply and demand for long-term assets, which, among its other virtues, did not rely on the assumption of a constant liquidity premium or a constant equilibrium return.

A number of other participants expressed their discomfort with the assumption of constancy of the equilibrium return, and particularly of the nominal equilibrium return. Mishkin responded that he did not establish that the equilibrium rate was constant nor did he believe that the liquidity premium was constant. He explained that he had conducted the tests reported in the text assuming constancy as a simplification. The results of the tests are not significantly affected so long as the variation in the equilibrium rate of return is small relative to other sources of variation. He noted that some of his equations implied that the equilibrium changes amounted to less than 2 percent of the total variation in bond returns and additional tests that allowed the equilibrium return to vary with the short-term interest rate did not lead to appreciably different results.

Brainard had another problem with the concept of equilibrium applied in the paper. He thought it necessary to distinguish between movements in interest rates that were required to maintain an equilibrium configuration among yields and movements that established a new equilibrium following some shock. The "paradoxical" negative relationship between various short-term interest rates and the stock market yield may simply reflect the fact that an increase, say, in short-term interest rates, requires a decrease in stock prices to establish a new equilibrium in which the expected return from stocks is higher. Given discrete sample periods, these adjustments to equilibrium complicate the estimation of the equilibrium relationships among asset returns. He also argued that the analysis ought to try to identify the cause of such shifts—for example, a change in inflationary expectations, a change in the marginal rate of time preference, or a change in expected profits or taxes—because various types of disturbances require different revaluation of assets to restore equilibrium.

Mishkin's simulation experiment with the MPS model provoked considerable discussion. Sims criticized the implicit assumption that policymakers could control people's perceptions and expectations in such a way as to convince them immediately of the permanence of any particular change in interest rates. In fact, a major uncertainty about any policy action is how it will affect expectations, and neither the policymaker nor the outside analyst can legitimately rule out that problem by assumption. Sims also indicated that, once the equation determining the long-term interest rate in the MPS model was changed, other equations in the model also had to be altered. For example, investment is linked to the long-term interest rate in the model. Although short-term interest rates also influence investment, their effect is normally picked up by the long-term rate. But in Mishkin's illustrative simulation, the long-term rate changes by an unusually large amount relative to the short-term rate, and hence the change in investment calculated from the model is likely to be exaggerated.

Robert Gordon saw other related problems in the simulation. He stressed that a surprising drop in the short-term interest rate that reflected an unusually expansionary monetary policy would have subsequent effects on both real activity and inflation that tended to drive interest rates back upward. Hence it seemed implausible to him that any policy easing of that kind would be interpreted in the market as a permanent lowering of interest rates, regardless of what the Federal Reserve said about its intentions. Moreover, he conjectured that any big surprise in Federal Reserve policy would be greeted with disbelief in the marketplace. Hence, market participants were unlikely to conceive of it as permanent and indeed might translate this change in the short-term rate less significantly and less rapidly into long-term rates. Gordon noted Mishkin's finding that, on average, a surprise in the short-term rate of 100 basis points is accompanied by a change in the long-term rate of 36 basis points; he doubted that a very different result from that average should be attributed to any particular hypothetical policy action.

Stephen Goldfeld was troubled by another feature of the simulation. It assumes that the expected path of interest rates is pulled down as a result of the policy action by 50 basis points below whatever path of rates was previously expected. If some changes in short-term interest rates were initially expected by market participants, the same changes are subsequently expected but from a benchmark level that is 50 basis points lower. Such an alteration of expectations was, in Goldfeld's view, "a strange animal."

Mishkin responded that he was in general agreement with these comments that caution against the reliability of the simulation results. The simulation experiments in his paper are not intended as a guide to policymakers, but are rather an illustration used to convey the following point: macroeconometric models that fail to impose financial market efficiency tend to overstate the lag in the effects on aggregate demand of changes in short-term interest rates resulting from monetary policy.